1. Review

Comment	Reply
Difference to previous fire models in LPJ, especially Reg-FIRM	This is a good suggestion, and helps to underline the improvements on fire modelling in the LPJ model family. We have added a paragraph to the discussion section to point out substantial improvements and new approaches in SPITFIRE. As can be seen there fire spread equations are based on the Rothermel BEHAVE equations, whereas Reg-FIRM used the simplified fire spread model from Telitsyn (1988, 1996). The Telitsyn equation is conceptually related to the Rothermel fire spread equation, but SPITFIRE needs further model details to simulate fire intensity and post-fire mortality from fire spread conditions – functionalities the Telitsyn equation does not provide. Unlike Reg-FIRM, the ROS equation in SPITFIRE incorporates variable wind speed, fuel moisture and fuel structure (see Fig. 2 for model overview). Tree mortality is no longer a constant as in Reg-FIRM, SPITFIRE models it much more biologically realistic as it depends on calculated fire intensity (not included in Reg-FIRM), fire residence time (not possible to calculate from Telitsyn equation alone) as well as height, bark thickness, and PFT-specific post-fire survival characteristics. This approach allows capturing fire-adaptation strategies (e.g. savannah trees withstanding full crown scorching) vs. high fire sensitivity of rainforest trees on the one hand, and considers complete scorching of small trees vs. tall trees, which escape the flaming zone.
Add analysis, why fire season length is overestimated in most biomes and underestimated in the boreal zone	We think that a more comprehensive analysis, using e.g. regional fire and vegetation data, would be required to fully address this question. We have added some text that explains what we think might cause these differences to give an outlook.
Add parameter list and add	A table containing the variables and parameters described in the

description of $U_{forward}$, when first mentioned.	equations 1-22 of the main section has been added as Appendix C. The explanation of the variable $U_{forward}$ was added to the text.
Difference between tau(l) and t_{fire}	The variable τ_l describes the residence time of fire in minutes that is the length of time flaming front passes a particular point and is used to calculate how long a tree is engulfed by fire which could potentially damaged its cambium. The residence time is driven by burning conditions of the spreading flaming front. The variable t_{fire} describes the fire duration in minutes, i.e. total amount of time a fire burns and spreads across the landscape dependent on weather conditions.

2. Review

Comment	Reply
Improve introduction with interpretation, where fire is natural component and where not	The reviewers is correct in this comment, additional text now reflects this point in this paragraph. However, the main aim of this paragraph is to emphasize the approach used to come up with current global biomass burning estimates and why these approaches can't be used for climate change projections.
Add chart to outline, what a good fire model should have and what SPITFIRE covers based on Fosberg et al. 1999, Keane et al. 2004 and e.g. Pausas et al 2004 for description of vegetation influence.	Corresponding text and chart has been added to the introduction section and is also discussed in section 3.6
Characterization of fire model as being process-based, influence of parameter values chosen	The model is process-based, i.e. the principal equations of the model describe explicitly the component processes, rather than describing outcomes in a statistical sense. In common with all process-based models (including climate models!) the model contains parameters

and stochasticity.	that are only approximately known so there is some degree of empiricism that can't be avoided when selecting values for them. We have performed a sensitivity test for the PFT-specific parameter values using the ranges of values found in the literature, and also for the ignition rates. We decided not to include this analysis in the manuscript because it would add unnecessarily to the length while still falling short of a full analysis of uncertainties, which would be a substantial study in itself. We do however cite literature sources in the text for all parameter values used in the current version of the model (some of the citations are in Annex 2, describing details of the fire spread model). SPITFIRE itself has no sources of stochasticity. Although fire incidence is treated as a spatial Poisson process, we disregard stochastic variation in the number of fires per grid cell; this is stated in the text. Thus, all processes in the fire module are treated as if they were deterministic. The only stochastic process of the LPJ model is the weather generator (Gerten et al. 2004) which distributes monthly rainfall according to the number of rain days to obtain daily rainfall. This is used as a criterion in the NI calculation. Corresponding text has been added to section 2.1 "Modelling principles".
Role of wind speed – why monthly instead of daily values used - and why topography left out	As a first step of the SPITFIRE model we used averaged monthly wind speed data from NCEP re-analysis data (see section 2.3). Average monthly wind speed data was used because we wanted this paper to focus on assessing whether the LPJ-SPITFIRE is robust enough to reproduce average global fire distributions, without being distracted by potential impacts of daily climate variability and extremes. Knowing that this is now the case, we can move forward to include daily wind speed. This point was added to section 3.6 to discuss potential limitations and area of improvements for SPITFIRE. It remains true that neglect of extreme high wind values could be a source of error, specifically with regard to the rate of spread of forest fires, and a possible contributory cause for the underestimation of

	burnt area e.g. in boreal forests. Topography is not represented in any DGVM to our knowledge, and this applies also to our (and other) models' simulation of fire just as it does to radiation regimes and runon/runoff regimes. It would be desirable to develop a way to represent topographic variation in DGVMs but this would be a large project. That said, the influence of steep slopes in increasing the rate of fire spread (which we neglect) could be another cause of underestimation of burnt area, especially in woody vegetation.
Expand description of FDI with emphasis on eq. 6 and 7	We include the 10-, 10- and 100-hour fuels into our weighting of dead fuel moisture (eq. 6), which is then used in the calculation of fire spread, based on the assumption (well supported in the operational fire literature) that these fuel classes are the ones whose moisture content determines the rate of fire spread. The 1000-hr fuel class (i.e. coarse woody debris) is partially consumed in fires but does not materially contribute to determining the rate of spread. Therefore, this fuel class is only considered in the fuel consumption routine (see Appendix B1). We added a sentence stating that we assume that atmospheric conditions described by the Nesterov index and the calculated fuel moisture determine fire danger in SPITFIRE. The forms of the equations used here imply no abrupt decline when the moisture of extinction is approached. We have added an equation describing the linear response of the probability of fire spread and show the expanded version of the FDI equation. This shows that there is a non-linear increase in fire danger once litter moisture falls below the moisture of extinction. We have added some text that helps to disentangle the processes involved in the description of fire danger.
Fire damage to plants – limitation by using North-American literature only.	The approach used to describe fire damage to plants is the first attempt (in a DGVM context) at an explicit representation of the different mechanisms involved. As such, it requires empirical information that is not yet available for a wide range of PFTs . So, we

	have taken it as a hypothesis that the modelling approach using parameter values obtained for one ecosystem type can be applied elsewhere. This is the best that can be done in the absence of more extensive information. The fact that the model gives credible results across different ecosystems is encouraging, but inevitably not conclusive; neglect of variation in the relevant parameters (related to PFTs) is a possible source of model errors. The Pausas 2004 study published in Ecology concerns crown/stand-replacing fires, which the present version of SPITFIRE does not model.
Fire danger not zero in all desert areas. Plot FDI vs. fuel load for a number of biomes.	We have added a figure displaying the temporal dynamics on a grid cell basis for selected biomes. These graphs display the different combination between fire risk, fuel load and resulting area burnt for desert, boreal, temperate and semi-arid climates. We think that this additional illustration improves the explanation in section 2.1.
Error of vegetation type or biomass for providing wrong input into SPITFIRE thus fire results, better discussion of results.	While section 3.1 aims to describe the global distribution of simulated fire pattern, sections 3.2, 3.3 and 3.4 describe the model evaluation of seasonal timing of fires, fire season length and burnt area (on qualitative terms) using Earth observation products. These sections already contained sentences explaining possible or known causes of mismatch. However, the new text we have added hopefully now describes better model limitations and potential errors arising from wrong LPJ or climate input. The reviewer phrases an interesting idea, where e.g. in a region-specific analysis reason of mismatch caused by wrong vegetation input from LPJ or climate could be elaborated in a separated study. We have stated this in section 3.6 discussing potential model limitations. In order to conduct such a study detailed fire data and EO or ground-based data on vegetation would be required. Please also note our reply to Reviewer 1, who raised a similar question on over- or underestimation of fire season length.

Limitations and assumptions of the SPITFIRE model	The reviewer is correct. We added some text to the discussion section 3.6. See also reply to other comments regarding discussion of LPJ-SPITFIRE results.

3. Interactive Comment by Sergey Venevsky

S. Venevsky has a number of comments, many expressed with great vehemence, and most incorrect.

Basic equation

He is right that P_d is not a probability (the new text clarifies this). It is a *probability per unit time*, a well-established physical concept with units of T^{-1}.

Eq (2) is correct. Both sides have units of ha d^{-1}, as could be deduced from the units given in the original text (l5-10 p 705).

Lightning ignitions

The reference is given for the estimate of 0.2 as the ratio of cloud to ground strikes. The way the references were cited meant to refer to the first part of the same sentence; this is now more precisely presented to preclude misunderstanding of the introductory text. Venevsky cites another reference for a modest range of estimates which are not much different from 0.2 (when considering the larger errors on other multipliers, notably the proportion of “effective” strikes).

The interannual variability of lightning strikes is, in almost all climates, much smaller than the seasonal variability. Venevsky cites a case of variation by a factor 2 in Canada, but the variation between summer and winter is 2-4 times larger (Christian et al JGR 2003).

It is true that the model as presently configured cannot simulate the interannual variability of lightning strikes, but it does *not* follow that the model cannot simulate the variability of lightning-started fires because this variability is strongly dependent on the interannual variability of climate, as numerous studies have shown.

Human ignitions

Venevsky’s remark to the effect that population cannot increase indefinitely is beside the point. The point is that empirical evidence, presented since the publication of RegFirm, has established that there is a (low) population density above which the rate of fire setting by humans declines. The original function did not possess this turning point.

Fuel moisture and fire danger

Equation (6) is dimensionally correct. The LHS is a dimensionless index between 0 (dry) and 1 (wet). The RHS is also a dimensionless index between 0 and 1. The NI is a cumulative value, which is set to zero on rain days, and accmulates faster on hotter and dryer days. In the case of constant weather, equation (6) describes a negative exponential drying curve, set to 1 on the rain day and declining thereafter, until the next rain day.

The α terms are set to give appropriate drying rates for the three fuel classes. We have now specified their values in the text. The *ratios* among the three α terms are set by the *ratios* of the surface-to-volume ratios imputed to the classes. Their order of magnitude is indeed small, set by the (arbitrary) numerical scale of the NI. Although their values might depend to some degree on plant types, there are few data available that would support such a dependence, and (most importantly) fuel particle size (independent of PFT) is universally agreed to be the single most important predictor of drying rate.

Venevsky's argument about errors is absurd – the fact that NI is large and the α terms small does not imply that this calculation is error-prone. This can be confirmed by a simple thought experiment in which the NI values are scaled down by a factor of (say) 10^3, and the α terms scaled up accordingly to give the same drying rates.

Equation (6) is in fact identical with equation (7) of Venevsky *et al.* (2002) except that the former supplies a physical interpretation for the α term in the latter, and furthermore allows this term to vary in a physically meaningful way according to the composition of the fuel.

It is true that the original LPJ code did not calculate the amounts of litter to be assigned to fuel classes. SPITFIRE does this. We now describe the proportion of vegetation carbon pools assigned to the respective fuel classes in LPJ in section 2.1.

Rate of spread and duration

Equation (13) does reach a maximum of 4 hours duration. Fire duration is determined by landscape structure, weather conditions during the fire and fire fighting. To address this correctly one would also need to consider several days of burning for a single event in the model and conditions associated with continued burning (including over-night "stand-still"). There is no well established function for fire duration, so this is an area that needs further work, especially in a DGVM modelling context. We have now noted this point in the text, see discussion 3.6.

Model evaluation

The comparison among all three parts of Fig. 2 provides useful information for understanding how the model works. We have not attempted to evaluate numbers of fires because this is not possible except for certain regions where field obervations exist. Number of satellite-derived fire hotspots is NOT equal to number of independent fires Also, minimum size of fire detected by different sensors, and their overpass rate affect fire hot spot data.

It is surprising that the validation of the model is said to be "almost non-existent". The model is tested against global remotely sensed data on both seasonal timing and area burned, and regional data on area burned and its interannual variability. The testing is far more comprehensive than was attempted, for example, for Reg-FIRM by Venevsky *et al.* (2002).

Fires *are* simulated in southern Siberia.

The paper refers to Giglio *et al.* (2006) for the method of calculating fire season length, which we applied consistently to both observations and model outputs and had stated this in the manuscript text.

Editing

We had intended to cite the SEVER-FIRE model in our manuscript, but only publications published in peer-reviewed literature can be cited. This is unfortunately not the case (ISI Web of science, April 2010, search term "Venevsk* AND fire"), also his personal webpage did not list a peer-reviewed article about SEVER-FIRE in April 2010. An old powerpoint presentation on the web is not a publication.

We disagree that the description of results is too long.